Review, J. Eppler at al. "Snow Water Equivalent Change Mapping from Slope Correlated InSAR Phase Variations"

General comments:

The manuscript provides a new method for estimation of snow water equivalent (SWE) based on repeat-pass SAR interferometry. Despite very promising results in specific cases, reliable estimation of SWE by SAR interferometry is an over 20 years old problem which could not be widely applied due to unknown phase offsets and ambiguities. The manuscript by Eppler et al. tackles this problem with a brilliant new idea. The authors demonstrate their method using an eight year long time series of Radarsat-2 SAR imagery.

The manuscript provides a clear description of methods, a very detailed analysis of error sources, and a validation of the method based on field measurements and model results. Despite beeing very radar-specific, the work is excellently suited for "The Cryosphere" because it adresses the important problem of SWE estimation which is done with a wide range of different sensors and methods.

Except for a list of small specific comments (below), including several suggestions to shorten the manuscript, I have two minor suggestions to make the method more clear and to improve the structure of the manuscript: 1) method: I suggest to better explain how the sketched 2D geometry is applied in the real-world 3D-geometry. 2) I suggest the discussion and analysis of error sources (section 5) be moved behind the result section and try to shorten section 5. This section 5 about error sources contributes to more than 25% of the manuscript and puts a long "barrier" between the method section and the seems to be better suited in the discussion part rather than between the methods and results.

We thank the reviewer for their constructive comments and suggestions for clarifying the manuscript and improving the structure. We have made all the suggested changes except for two: (1) regarding the suggestion to add a comparison figure for all bias sources; and (2) regarding the suggestion to quantify the coherence of snow free areas.  Please see below for our rationale and response to each comment (colored in blue text). Note that the manuscript has been significantly revised and therefore some of the section, figure and equation numbers have changed.  Our responses refer to the numbers in the reviewed version of the manuscript.

Specific comments: --- Abstract ---line 14: "RADARSAT-2": It might be good to mention C-band.

Agreed.  The text has been changed to "…RADARSAT-2 (C-band) …"

--- Introduction ---line 63: You could add here two references to polarimetric methods to estimate quantitatively the amount of fresh snow. These dual-pol approach could, possibly, be used to provide complementary, non-terrain-dependent information about

SWE changes to you method (in case a dual-pol radar is available). See https://doi.org/10.5194/tc-10-1771-2016 and https://doi.org/10.5194/tc-14-51-2020.

We thank the reviewer for these references. They have been included in the following text "*Furthermore, polarimetric refraction-based methods have been proposed to exploit the structural anisotropy of snow to provide additional information about SWE change within a snowpack (Leinss et al., 2016, 2020), although in this article we focus on single-polarimetric methods*."

line 87-90: Could you add here half a sentence more to explain the "secret" of your method? Up to here, I have seen several promises and the key-ingredient of topographic variations. But half a sentence more of details might be worth to add. Something like "our method exploits the sensitivity/dependency of the signal/phase delay within the snowpack with respect to the local terrain slope".

We thank the reviewer for this suggestion. The following text has been added:

*"Our method exploits the dependency of the refractive phase delay within the snowpack with respect to the local terrain slope."*

--- Section 3: Method/Priliminaries ---line 150-160: general comment to these lines (see also the three specific comments follow below): In a quasi-2D coordinate system, these lines are convincing. However, in 3D-space, more precise definitions of angles are required. Please also define the coordinate system. I guess, Figure 3 and the definition of incidence angle are not defined perpendicular to the orbit direction but in the plane defined by the line-of-sight and the surface normal of the topography. Such a consideration, especially with respect to the geometry shown in Fig. 3, could require to consider refraction into the dimension of the orbit direction, e.g. for slopes where the surface normal vector has a component into the orbit-direction. Theta and alpha might not be located in the same plane.

We thank the reviewer for these comments. These comments are consistent with our treatment of the geometry. We agree that the text should be revised to provide additional clarity regarding the geometry. A couple of points:

(1) As noted by the reviewer, Fig 3 depicts the plane which includes both the SAR line-of-sight vector and the local slope normal (n). As such, it is not a special case, but is always fully correct regarding the refraction geometry. The refracted ray path will always occur in this plane. The local incidence angle is the angle between the reversed SAR line-of-sight and 'n' and is therefore correctly depicted in this diagram.

(2) The plane depicted in Fig 3 does not, in general, include the local zenith vector, and therefore theta and alpha are not generally located in the same plane. Figure 4 does depict them in the same plane for illustration purposes but this is a special case. This may be misleading and therefore additional text has been added to the caption of Fig. 4 for clarity.

Figure 3 might gain value by adding a small 3D-sketch indicating how the two dimensions of the current figure 3 are defined. The figure caption should also explain the orientation of the shown 2D image in the 3D space.

A small 3D sketch has been included as an inset to Figure 3 showing the general 3D geometry and how alpha and theta angles relate to defining vectors $\boldsymbol{z}$, $\boldsymbol{n}$ and $\boldsymbol{l}$ (SAR line-of-sight direction). The figure caption has been revised include the following text: *"The inset figure shows the general 3D geometry. x, y and z refer to the local East, North and vertical directions.* $\boldsymbol{n}$ *and* $\boldsymbol{l}$ *refer to the local slope normal and SAR line-of-sight directions which, together, define the plane depicted in the 2D diagram."*

line 153: "local incidence angle theta": Comparing the derivation of Eq. (1) with Figure 3, I guess the local incidence angle is measured with respect to the terrain normal n. To avoid confusion with the "local incidence angle" with respect to e.g. the ellipsoid, I suggest to clearly state with respect to which direction (e.g. terrain surface normal) theta is defined. I suggest to also add, that such a definition makes theta also dependent on the aspect of the slope.

We agree. The following has been added: *"Note that θ is defined as the angle between* ***n*** *and -**l** (i.e. reversed SAR line-of-sight) and therefore depends on both the magnitude and aspect of the local slope."*

--- Section 4: Method ---In line 262 you speak about "aspect angle maps". I guess, it could make sense to introduce them here in line 150-160.

The following text has been added: *"As such, this geometry can be defined everywhere within a SAR scene given maps of spatially varying **l**, expressed in the local (East, North, vertical) coordinate system and DEM derived slope magnitude and aspect maps."*

line 156: "alpha is the local slope angle": How is alpha defined in the 3D geometry?

It is the angle between the slope normal and vertical directions. The text has been changed to *"…where α is the local topographic slope angle, defined as the angle between n and z."*

line 185: What is the unit of xi? rad/mm? or rad/mm SWE. It might be good to add a sentence about how much xi varies over a certain range of slope, e.g. for slopes between -30 and +30 degree, rho=0.3, lambda=... xi varies from 0.22(?) to 0.28 rad/mm.

We use "mm" rather than "mm SWE" throughout the document as the unit for SWE. Therefore, for consistency, $\xi$ has been expressed in units of "rad/mm".

line 215: "Assuming that the first term ~xi is the dominant component": Could you provide some argument for this assumption?

On reflection, our wording is misleading/incorrect. For example, when <ΔS> is zero then the first term will be zero, and certainly not dominant. We were trying to say that the additional terms contributing to the bias need to be sufficiently small. This is already expressed in the subsequent paragraph, *"The estimator relies on the assumption that the horizontal SWE change distribution is uniform within the window and that other interferometric phase components are uncorrelated with $\tilde{\xi}$".* The text has been changed to *"Assuming $\tilde{\Phi}$ correlates with $\tilde{\xi}$ with the proportionality <ΔS>, we introduce the following correlation-based estimator for <ΔS>"*. See also, the next comment.

line 215: could you add: "... and that ~Phi correlates with ~xi with the proportionality <\Delta S>"

The suggested change has been made. See the previous comment.

line 261: "interpolation artifacts": where would they come from?

The use of "interpolation artifacts" is a poor word choice. We meant "interpolation error" which is simply the difference between the interpolated value and the true but unknowable value at the interpolation point. The text has been changed.

line 262: "artifacts from the cubic interpolation": (bi)cubic interpolation is known to cause overshoot. Why did you not use e.g. bilinear interpolation?

We did not thoroughly investigate the issue of most appropriate interpolator and so there may be room for improvement here. The following text has been added: "*We did not thoroughly investigate the issue of most appropriate interpolator and so it may be possible to reduce these errors by using a different interpolator*."

Figure 7: What are the uncorrelated phase components? Could you provide a variable name?

This refers to the phase components discussed in the section titled 'Uncorrelated Phase Components'. A forward reference to the section has been added to the figure caption.

line 290: "normalized range bandwidth": is that bandwidth / central frequency?

Yes, the text has been changed to *"bandwidth of the SAR normalized with respect to carrier frequency"* for clarity.

--- section 5, Error sources ---Would it be possible to summarize all the errors discussed in the whole section 5 in a figure? Something with a caption line "Estimated magnitude of SWE errors through the estimator due to different error sources".

This is difficult because our approach to the error analysis has been to estimate the sensitivity of the SWE errors to particular factors which may vary significantly both in time and spatially within the scene (e.g., heave magnitude, solifluction rate, decorrelation). Furthermore, for soil moisture phase, we only derived an upper bound to the error. For these reasons it is difficult to provide such a summary of their relative contributions.

line 378: "to detect these events by analysis of the wind history": Would an analysis of SAR data from a different orbit direction cause an error with the same sign or would the errors average out? i.e. could a parallel observation from the opposite orbit direction also be used to detect such events?

This is an interesting question. Yes, since the effect is dependent on sensor flight direction, comparing results from differing geometries should allow for detecting this effect since the SWE change estimates should then differ. The effect should reverse sign for near-parallel but opposite flight paths so averaging such paired results would mitigate this effect to some degree. However, due to orbit inclination, it is not possible to achieve this parallel condition exactly, especially at higher latitudes. The following text has been added:

*"Another potential mitigation is to use SAR images from the opposite pass direction. The bias from the near-opposite horizontal direction should have the opposite sign and therefore the estimated $\Delta$SWE should differ significantly between pass directions, allowing for the effect to be detected and perhaps mitigated by averaging the results."*

line 415: Describing the "static" component as a "horizontal mean component" appears confusing to me. Especially because the "horizontal mean component" is "modulated by topography". So, "horizontal mean component" might require some rephrasing, indicating where the modulation by topography comes from. I guess, static means related to the density of different horizontal air layers or simply different air pressure or humidity. Maybe, simply drop the words "horizontal mean component and horizontally variable component" and directly call them static and dynamic.

Agreed. The text has been changed to be simply: *"This delay can be decomposed into static and dynamic components."*

Section: 5.3.2 This section could be slightly shortened.

We have shortened this section by removing about seven lines of text.

line 466: "summer interferograms can be used to identify areas": As you describe, heave and subsidence are periodic, hence, in theory, observation of subsidence could be used to estimate the error due to heave.

Yes, good point. The following text has been added: "*It may also be possible to estimate and correct for the early-winter heave component by estimating the thaw-subsidence from summer-season interferograms and then applying a model for the cyclical deformation.*"

section 5.3.3: Could be shortened.

We have shortened this section by removing about six lines of text.

section 5.4: could be shortened.

The part on Monte Carlo simulation has been slightly shortened and moved to the methods section. The rest of this section has been significantly shortened and moved to the start of Section 5.3. As a result, this section has been removed from the manuscript.

line 522: To make it easier for the reader to find where you describe the Monte Carlo estimations, I suggest to add here a sub-sub-section heading.

Agreed. A sub-sub-section heading has been added.

section 5.5: can be shortened.

Eq. (28) and several lines of text have been removed.

--- section 6: Results and Discussion ---585: "the in situ measurements correspond to an upper limit" - considering the many positive biases due to various error sources, I'm not sure if that's true.

This is a good point. The text has been revised: *"Therefore, for the comparison, the in situ measurements correspond to an upper-limit for the expected SlopeVar estimated total SWE maps, neglecting any biases. However, given the error sources described in Sect. 5, actual estimates may exceed this expected upper-limit."*

592: "sampled at the spatial mean position": Looking at Table 2, it seems the resolution of the estimator is not good enough to compare individual SWE samples with individual estimated values. It would be good to refer to "the transect length given in Table 2" to justify why the spatial and SWE mean values were used for comparison of measurements with estimated values.

Agreed. The following text has been added: *"This was done because the transect lengths, listed in Table 2, were 200 m or less, being well within the 500 m SlopeVar estimator window size."*

634: "likely, because only snow-free areas (..) are coherent": I think this should be easy to check to make a better confirmed statement. Something like: "most melting snow areas have a coherence below ... which are not considered in the estimator." (check that the method section contains the information how to deal with low-coherent areas).

We disagree with the reviewer on this point because it is not straightforward to identify ‘melting snow areas’ as spatial subsets within each interferogram. One could attempt to estimate these based on the coherence magnitude itself but then the resulting coherence estimate for melting areas will depend on the chosen threshold.

The ‘Estimator Implementation’ section has been revised to include information regarding dealing with low-coherent areas:

*“Solutions where the maximum was within 2 grid intervals from the edge of the search range were labeled as invalid. This provides a means for excluding poor results from low coherence areas since for these areas the periodogram analysis typically does not result in a peak within the search range. We considered adding an additional exclusion threshold based on coherence magnitude but found it unnecessary. Water-body areas were also labeled as invalid.”*

--- section 7 ---I don't see the relevance of section 7 for the paper. This section could well be published as a seperate contribution/letter. I suggest to remove at to make the paper more concise.

We included this section in an effort to appeal to a wider readership and provide a demonstration of how the method might be applied but recognize that it adds additional length and can be removed without affecting the method presentation which is the primary goal of the manuscript. The section has been removed and we will consider including the analysis in a separate publication.

--- section XX: discussion ---The "result and discussion section (6)" has only a few references to section 5 (error sources). However, the 10-page long section 5 puts a significant barrier between the method and the result section. Therefore I suggest section 5 be moved behind the result section. An exception might be the paragraph after line 522 (monte carlo approach) which could be incorporated into the method section.

The manuscript has been significantly restructured. Former Sections 3 and 4 have been incorporated in a single ‘Methods’ section (now Section 3). The results from ‘Results and Discussion’ have been put in the ‘Results’ section (now section 4) and the error sources (former Section 5) have been merged with the other discussion subsections into a ‘Discussion’ section (now Section 5). The subsection on Monte Carlo approach has been moved to the methods section.

line 715: "DEM-derived dry-snow phase sensitivity map" - I would add "[DEM-derived,] slope-dependent..."

The suggested change has been made.

--- conclusion ---

line 718-730: Similar to my sugggestion regarding section 5, I suggest this paragraph be moved behind line 741. I also suggest to make this paragraph as compact as possible.

This paragraph has been moved and shortened significantly.

line 752 - 755: similar to section 7, I suggest to remove these analysis. Line 742-751 provide a good finish of the manuscript. (check also line 18-20 in the abstract).

Agreed. These lines, and those in the abstract relating to section 7 have been removed.

Technical corrections:

1-6: "as an alternate technique": I guess you mean "alternative". You could also start with "Another option is repeat-pass ..., that allows"

Agreed. The text has been changed to: “Another method is repeat-pass … that allows…”

line 149 "the phase of the SAR signal" -> "the unwrapped phase of the SAR signal" (to define $\Phi$; see also comment below for line 235.)

The suggested change has been made.

line 171: "horizontally uniform": Could it be better to say "spatially uniform"? I know what you mean by "horizontaly uniform" but it might be confusing to first read "constant topographic slope" and then "horizontally".

Agreed. The suggested change has been made.

line 174 "dry-snow snow" -> "dry-snow"

The suggested change has been made.

Figure 4, caption: "While vertical snow depth is constant" -> "While vertical snow depth $Z_s$ is constant"

The suggested change has been made.

Figure 5, caption(b): "Topo-corrected 24-day interferogram": Specify, if this is an unwrapped interferogram.

The shown interferogram is wrapped. This has been added to the caption.

line 232/234: "alternate" do you mean alternative, altered or modified? I understand alternate as swapping back and forth.

Agreed. The suggested change has been made.

line 235: "the set of wrapped phases" -> "the set of wrapped phases $\phi$" (makes it easier to follow the argument that exp(j*phi) = exp(j*Phi).

The suggested change has been made.

great work!
**Citation**: https://doi.org/10.5194/tc-2021-359-RC1

The Cryosphere: Eppler et al., Snow Water Equivalent Change Mapping from Slop Corrected InSAR Phase Variations

**General Comments:**

The study presented attempts to quantify snow water equivalent (SWE) using interferograms of wrapped phase from 9 years of RADARSAT-2 acquisitions over the Trail Valley Creek region of the Northwest Territories. The authors present a clear and sound scientific analysis of the interferometric principles and how they apply to snow overlying a variable topography with underlying tundra/shrub landcover classes. In essence the study is a significant contribution to the development of snow water equivalent retrievals using spaceborne SAR, especially C-band for relatively shallow snowpacks because it is generally understood that the snow depth/grains in tundra regions are too shallow/small to produce significant volume scatter, respectively. The understanding of the signal interaction with the snow depth and volume is well-presented, and is valuable for those entering this research space.

That being said, the theoretical construct of the paper to retrieve change in SWE (ΔSWE) hinges on the assumption of a consistent snow density across the study terrain, as well as year over year. As a reader this presents as problematic because in Section 6.2. the in-situ transects are presented, but the snow density is described is 0.3g/cm3 across the study region and times in the Winter season. In addition, there are only two years in which snow observations of the snow properties are incorporated into the analysis. There have been extensive observations of snow depth, density, and influence of vegetation going back to 2012 by Environment Canada, and it would be useful to see this incorporated into the understanding of snow density. Overall, the reliance of a bulk snow density also does not incorporate the reality of snow conditions in tundra regions of Trail Valley Creek, where snow is commonly a combination of a wind slab and depth hoar layer, with high and low snow densities, respectively. Conceivably, this could also change the signal interaction with the snow volume, as refraction and velocity would be slightly modified. This is not addressed as a limitation.

We thank the reviewer for their constructive comments and suggestions for clarifying the manuscript and improving the structure. We have addressed all comments and, in most cases have made the suggested changes.  However, we disagree with the reviewer regarding several of the comments, most notably:

(1) those regarding the importance of prior knowledge of snow density and the suggestion that the proposed method is better suited as a depth estimator
(2) the suggestion that we obtain data from the Trail Valley Creek site to validate the results

Please see below for our rationale and response to each comment (colored in blue text). Note that the manuscript has been significantly revised and therefore some of the section, figure and equation numbers have changed. Our responses refer to the numbers in the reviewed version of the manuscript.

Some more general comments before specific comments:

- In Section 5.1. you discuss how snow density changes due to "settling". It's important to note that the density within the snowpack varies as well. Bulk density can be used commonly in these types of analysis, but it seems uniquely important here to address that the wind slab and depth hoar densities are quite different.
  - Several studies have also reported snow densities for this regions and study period, for example (among others):
    - Rutter, N., Sandells, M. J., Derksen, C., King, J., Toose, P., Wake, L., ... & Sturm, M. (2019). Effect of snow microstructure variability on Ku-band radar snow water equivalent retrievals. *The Cryosphere*, *13*(11), 3045-3059.
    - King, J., Derksen, C., Toose, P., Langlois, A., Larsen, C., Lemmetyinen, J., ... & Sturm, M. (2018). The influence of snow microstructure on dual-frequency radar measurements in a tundra environment. *Remote Sensing of Environment*, *215*, 242-254.
    - Meloche, J., Langlois, A., Rutter, N., Royer, A., King, J., & Walker, B. (2021). Characterizing Tundra snow sub-pixel variability to improve brightness temperature estimation in satellite SWE retrievals. *The Cryosphere Discussions*, 1-22.

      We thank the reviewer for the references and have included citation of Rutter et al (2019) and King et al (2018) below. Regarding wind slab, reviewer #3 asked a similar question regarding the commonly encountered wind slab over hoar profile. However, it can easily be shown based on the density misspecification analysis (i.e., Eq. (19)) that such a situation (even 500 kg/m$^3$ wind slab over very low density hoar) results in only a 2.5% estimation error when assuming $\rho = 0.3$. We have added the following text to the end of the section titled "Snow Density Misspecification":

      *"Regarding the effect of vertical density layering, our study area is prone to wind slab formation where the late season snowpack can consist of dense wind slab overlaying a low density hoar layer (Rutter et al., 2019; King et al., 2018). Considering the extreme case of near zero density hoar overlain by $\rho = 0.5$ wind slab, assuming uniform $\rho = 0.3$ results in a +2.5% estimation bias which is still a small error compared to the other bias sources considered in our analysis."*

The paper overall reads somewhat like a dissertation rather than a manuscript. Sections do not necessarily flow like a common manuscript (Intro/Background/Data/Methods/Results/Discussion), rather segmented into several smaller sections. This is more of a comment than requesting a change. For example, Section 3 (Spatial Variations of Repeat-pass InSAR Dry-Snow Phase), Section 4 (Estimation Method), and Section 5 (Sources of Estimation Error) – are these all sections within the Methods?

We thank the reviewer for this comment. The manuscript has been significantly restructured into the following sections:
1.Introduction
2. Data
3. Methods (contains both section 3 and 4 from the reviewed version)
4. Results
5. Discussion (contains most of section 5 from the reviewed version as a discussion of errors)
6. Conclusions

- In terms of validation, were no snow depth or SWE large scale transects ($n > 100$) used in this study? I understand that the exact snow depth or SWE could not be collected for every location or date, but as it reads we are accepting that the SnowModel outputs are truth and validating against that?

In situ validation was limited to the set of eight late snow seasons snow tube transects obtained in 2017 and 2018. We also used the ERA5 data as a temporal validation to assess the bias in the estimates.

We did not use SnowModel for validation but instead just used it to investigate the magnitude of errors caused by spatial SWE change inhomogeneity.

- Overall, I am slightly confused as to why the authors are presenting this study as change in SWE, because SWE is dependent on the depth*density. The authors are prescribing density across the whole study, during the entire season. Therefore, what they are truly retrieving is the snow depth. When the authors are attempting to quantify bias to SWE from many sources, they present in mmSWE, when as I understand it, they are actually quantifying change in snow depth.

We respectfully disagree with the reviewer regarding this comment and the other comments in this review which incorrectly stress the importance of densities for the proposed method.

The method does require a prescribed density as an input. It is also true that regarding the three quantities: density, depth and SWE, knowing any two allows the third to be determined. These two facts do not imply that the method is retrieving depth instead of SWE.

The opposite is in fact true: the method retrieves SWE and requires the assumed density to infer depth. As such, the method is well suited as a SWE estimator but only suitable as a depth estimator if the density is well known. The reason for this is that the estimator is quite insensitive to density misspecification. Section 5.1 (Snow Density Misspecification) covers this in some detail. Furthermore, Leinss et al 2015 discuss this in some detail, reaching the same conclusion which is well summarized by their Eq. (18). They state in their conclusion: *"A sensitivity analysis with respect to snow density and incidence angle showed a very weak dependence on snow density."* Our analysis, described in Section 5.1, agrees with this conclusion regarding density.

- Section 6.4.: The discussion about the active layer of the ground surface promoting a bias underscores how this paper could be improved by looking to quantify snow depth change as opposed to SWE (with SWE being inferred after using apriori knowledge of density). That way, the heave associated with the freeze could be compensated for within a snow depth algorithm, the same way that freeboard could be for lake/sea ice. I would suggest that presenting the change in snow depth as opposed to SWE would make Section 6.5. more straightforward to account for.

  We respectfully disagree with the reviewer. First, as noted in our response to the previous comment regarding depth, our proposed method does not measure snow depth. It is true, that if density is well known, then depth could be inferred from the SWE estimates.

  Furthermore, our method is based on differences in the repeat-pass propagation phase, not differential measurements of topographic height which is what this comment is assuming.

- While interesting, it's my feeling that the inclusion of Section 7 is too much for this study. There are new datasets, models, methods, etc., that are introduced and it should be a standalone study. The authors portend as much, stating on line 682 that it is not within the scope of this paper.

  Reviewer #1 made a similar comment. We have removed Section 7 and will consider submitting it for a separate publication.

Specific Comments:

Page 6 Line 140: “Spatial Variations of Repeat-pass InSAR Dry-Snow Phase” – is this the beginning of the Methods section? Or a Background section?

We have added the follow at the very beginning of this section to clarify: *“This section begins with a brief background on the InSAR phase contribution from dry-snow on a uniform slope and then extends this to the more general case of slope varying terrain. Together these describe the source of the spatially varying InSAR phase which our proposed method exploits.”*

Page 14 Figure 7: The right y-axis label for frame (d) says mm SWE – I believe this should be “Change in mm SWE”.

We were inconsistent with our use of units regarding SWE and change in SWE, sometimes using “mm”, and sometimes “mm SWE”. We have gone through and changed both absolute and relative SWE units to be simply “mm”. In this specific case the ΔSWE standard deviation unit has also been changed to “mm”.

Page 15: Section 5 “Sources of Estimation Error” = Should this read “Sources of Estimation Error in the Proposed Method”? It currently reads as a Discussion before the Discussion section.

Reviewer #1 made a similar comment. We have moved this section to after the discussion section.

Page 16 Line 309: “which as shown in Eq.(3), depends on snow density” – Yes I agree-this is where in-situ observation would be useful, for within the winter season or year over year.

However, the section containing this line then goes on to show that the sensitivity of the estimated SWE to prescribed density is low. Please refer to our response to the previous comment regarding this issue.

Page 17 Lines 346 – 348: “Snow Model, implemented….” – This is the first that I’m reading of the incorporation in the snow model, and this is Section 5 (which I’m not sure if it’s the Methods section or not). If this is being used for validation, it should be discussed in the methods section earlier on, with the model runs, input data, etc., specified. The new methods are continued to be presented until line 363, which may mean that these new methods need to be restructured into an earlier section of the paper.

The snow model was not used for validation. It was used to investigate the likely magnitude of bias contributed from horizontal SWE change inhomogeneity. This section has been moved to the discussion along with other subsections discussing bias sources affecting the results.

Page 18 Figure 10: What is the high end label for frame (f) on the x-axis?

The ticks on this axis were too sparse. We thank the reviewer for pointing this out. The high end of this axis is ~20 mm. More ticks have been added to the axis to make this clearer.

Page 19 Line 402-405: I know that I recommended that Section 7 be removed, however it would be interesting to note what landcover type elicited the most error within going into too much detail.

It is a bit unclear what the reviewer is asking for here since Section 7 and Section 5.2 (containing lines 402-405) discuss different things. Section 7 summarizes an analysis of the estimated SWE changes integrated by land cover type and basin footprint whereas Section 5.2 reports on modelled biases due to spatially varying snow holding height.

If the reviewer is referring to the subject of Section 5.2, then we should point out that the land classes are input into the SnowModel as a single snow holding height per class rather than a spatial distribution and so their effect on the modelled error (correlation between $\xi$ or $\xi^2$ and the modelled SWE over each estimation window) comes from the spatial variation in land class rather than the land classes themselves. With this in mind, we do not think it useful to report on the modelled error per land class.

If the reviewer is referring to the subject of Section 7, we do not have spatially continuous validation data and so cannot determine the estimation error per land class.

Page 23 Section 5.3.4: I don't understand this inclusion – how is this error potential derived with respect to soil moisture if there is no soil moisture data presented?

We have presented a theoretical argument to place an upper bound on the error. Such an argument requires no data to be presented.

Our presented SlopeVar method is an interferometric method and hence uses the repeat-pass InSAR phase. Soil moisture is a potential error source because of its InSAR phase contribution. The cited papers (De Zan et al (2014) and Rabus et al. (2010)) have presented results that show that there is an upper bound to this phase contribution. We have presented a 'worst case' scenario, i.e. assuming the upper bound soil moisture phase contribution and perfect correlation with the SlopeVar $\xi$

factor. Even for this worst possible case, we have shown that the resulting bias for the SlopeVar estimator is relatively small (i.e. 1.3 mm SWE).

Page 26, Section 6: Sections 3 – 5 were an extensive description of the methods (and could conceivably be truncated and merged into a single section for clarity), and we're getting to the results of Page 24 of the paper. My concern here harkens back to my comment that the paper reads more like a thesis dissertation than manuscript, because the Results and Discussion (including Section 7, which I believe should be omitted) only take up 10 pages, and is the most impactful portion of the work.

We thank the reviewer for this suggestion to improve the flow of the manuscript. We have made efforts to reduce the length of the error discussion and have moved it to the discussion section.

Page 26, Section 6.2.: "Comparison of SWE estimates with In-situ Measurements" – This information and data needs to be presented in the Data section. You provide the description of the different transects in Table 2, without listing what the values actually are- what are the snow depths? Snow densities? You state that you conducted these measurements with an ESC-30 snow density sampler, instead of listing a mean bulk density for instance.

We thank the reviewer for pointing this out. We have added a plot of all sample snow depths and densities for all eight transects and have revised the transect summary table (formerly Table 2) to include mean transect depth and density for each transect. Also, we have moved this description of the transect data to the data section.

Page 27, Lines 605 – 606: "SWE change predicted by the ECMWF ERA5 reanalysis model over the same time interval". Now, in the Results section, we are introducing a new data variable, one that has a km scale resolution, which is surprising for the reader. The ERA5 model spatial resolution is 9 km, meaning that the variability that is so crucial to this study is lost. You show one data point for each winter season to compare to the ERA5, so you are averaging spatially, and over time. There are existing snow depth and density records that have been extensively collected over Trail Valley creek, and I encourage the authors to reach out to those authors to obtain validation datasets.

We thank the reviewer for their comments. However, they are a significant mischaracterization of our study as conducted and described in the manuscript. ERA5 was used as a means of assessing the bias in the SlopeVar estimates and spatial averaging is appropriate for such a bias assessment. Furthermore, we disagree with the statement that ‘one data point was used per winter season’. In fact, 46 snow-season maps over 10 winter-seasons (2 partial and 8 full) were used in the snow-season portion of the analysis, corresponding to, on average 4 or 5 data points per winter season. We also disagree with the statement that the results were averaged over time. No averaging over time was conducted for this comparison which compares 24-day interval SWE change estimates.

Regarding the idea of validation with Trail Valley Creek datasets, our dataset image footprint is centered over the town of Inuvik which is 43 km south of the Trail Valley Creek site. Furthermore, Trail Valley Creek is situated above the treeline whereas Inuvik is below the treeline. For these reasons it is unclear how applicable Trail Valley Creek datasets would be to our study or even how they could be used for validation since there is no spatial overlap between the two sites.

Page 28, Figure 14: This graph presents a lack of detail based on the output of the analysis. What about histograms of change in SWE, to reflect the distribution of the data? Or statistical analysis of the in-situ vs slopevar estimator? For how exhaustive the methods and error source documentation was, the results here compared to in-situ data seem to be glossed over.

We thank the reviewer for these suggestions to improve the description of the results. Regarding statistical analysis of the in-situ vs SlopeVar estimator, there are eight transects and each is summarized by a mean SWE and computed standard error. These we compared to spatio-temporal interpolations of the accumulated SlopeVar estimates, currently displayed as a scatter plot with x&y error bars and we have quoted a computed global RMSE of 15 mm. We considered conducting a p-value based analysis of the hypothesis that the SlopeVar estimates are consistent with the in situ values assuming Gaussian error statistics. The problem with this is that the SlopeVar time-accumulated values ‘miss’ early season snow accumulation and therefore the in situ values represent an upper-bound for the SWE change captured by the SlopeVar estimates. We have additionally computed the bias (mean difference between the in situ and SlopeVar SWE values) and added it to the text: “*Treating the transect mean values as truth, and neglecting the unaccounted early snow-season SWE, the RMSE for all transect comparisons is 14.8 mm and the bias is -6.6 mm.”.*

Regarding histograms of SWE change, we did include these in Section 7, Fig. 17a, partitioned according to aggregated land cover class but these have now been removed along with all of Section 7 as requested. We have recomputed similar histograms but partitioned according to the {‘Oct-Dec’, ‘Jan-Mar’ and ‘Snow Free’} temporal subsets and added these to Fig 17 (Fig 16 in the reviewed manuscript).

Page 29, Table 3: Looking at the subset for seasonality, are these averaged over multiple years? Or just years with in-situ data? How does the averaging of multiple snow seasons together affect the results?

These seasonal subset statistics include data from all years spanned by the dataset. However, they are not computed by first averaging across the years. For example the ‘RMSE’, ‘Jan-Mar’ table cell is the RMSE of all (SlopeVar_spatial_average – ERA5) values computed over the set of all intervals occurring between 01-Jan and 31-Mar of each year.

General comments:

The manuscript entitled "Snow water equivalent change mapping from slope correlated InSAR phase variations" presents a novel method for the estimation of SWE change in dry snow conditions between repeat acquisitions using repeat-pass SAR interferometry, demonstrated using a RADARSAT-2 dataset (5.405 GHz) focusing on the region surrounding Inuvik, NT, Canada. This method leverages topographic variation and avoids the problem of phase unwrapping which has challenged previous InSAR studies.

The manuscript is generally well-written, and the methods appear sound. There are some issues with clarity throughout, especially with respect to the introduction, definition and use of symbols and expressions. Similarly, some of the figures appear too small and hard to read. Finally, stronger support from references is needed to improve the manuscript and provide context for the study, especially in the introduction.

I will provide general questions, specific questions by line number, and technical corrections in (mostly) chronological order, in the following sections.

We thank the reviewer for their constructive comments and suggestions for clarifying the manuscript and improving the structure. We have tried to answer each question and address each issue identified. The only exception is specific comment #11 regarding Fig1d and the requested aggregation of surficial classes. Please see below for our rationale and response to each comment (colored in blue text). Note that the manuscript has been significantly revised and therefore some of the section, figure and equation numbers have changed. Our responses refer to the numbers in the reviewed version of the manuscript.

General Questions:

1. Why did you choose the Inuvik area for this study? This wasn't addressed in the manuscript. You mentioned on Line 749 that you expect SlopeVar to perform better in high-relief areas and in areas where SWE > 150 mm, so it seems there must be more appropriate regions for this study given its reliance on topographic variation.

   We thank the reviewer for pointing this out. The following text has been added to the introduction: "*The study site was chosen for three primary reasons: (1) relatively long dry snow season; (2) moderate topographic variation, e.g., more than flat prairie-like conditions but less than in an alpine region; and (3) dataset availability since an archived stack of 120 Spotlight images over 9 years is somewhat unusual and allows for detailed temporal analysis of results.*"

2. Did you provide a discussion about where this method should be used, in geographic terms? Pan arctic? subarctic? alpine? I think you mentioned it should be used in areas with topographic features, but in terms of the Canadian landscape, where would this work or not work?

   This is a good question. The following text has been added to the discussion subsection,now titled “Spatial Resolution and Geographic Suitability”:

   “*These considerations affect the geographic suitability of the method, it requires dry-snow conditions and at least moderate topographic variation. Such conditions are present, at least part of the time in many geographic regions. The method is less suitable in areas with frequent wet snow conditions such as coastal areas and in regions that are mostly flat, such as prairie.”*

Specific Questions:

1. Line 13 – include the frequency, even if its in parenthesis.

   The suggested change has been made.

2. Introduction – You need to mention the study site and explain why you chose it. What is the significance of this site and why did you not choose other sites with more topographic variation?

   See response to general comment #1.

   Further, we wanted to test the method in an area with moderate topographic variation since this type of terrain is more commonly encountered in non-alpine areas.

3. Lines 26 – 28 – references needed – provide references for each point mentioned. This is important since it is setting the context for your entire study and should not be neglected.

   We have added the following references:

   T. P. Barnett, J. C. Adam, and D. P. Lettenmaier, “Potential impacts of a warming climate on water availability in snow-dominated regions,” *Nature*, vol. 438, no. 7066, pp. 303–309, Nov. 2005, doi: 10.1038/nature04141.

   M. Zemp, M. Hoelzle, and W. Haeberli, “Six decades of glacier mass-balance observations: a review of the worldwide monitoring network,” *Ann. Glaciol.*, vol. 50, no. 50, pp. 101–111, 2009, doi: 10.3189/172756409787769591.

J. R. Mackay and D. K. MacKay, “Snow Cover and Ground Temperatures, Garry Island, N.W.T.,” *Arctic*, vol. 27, no. 4, pp. 287–296, Jan. 1974, doi: 10.14430/arctic2885.

3. Lines 28-29 – may be worth noting that SWE is a function of depth x density as it will help uninitiated readers link SWE with commonly measured parameters.

   We added the sentence: *“The SWE of a snow layer is equal to the product of its depth and mean density.”*

4. Lines 30 to 34 – references needed – these are very dense sentences that are setting up the need for your study. Provide references for studies which demonstrate some of the challenges listed (eg. influence of topography, vegetation, and temporal bias).

   We have added the following references:

   T. Grünewald, M. Schirmer, R. Mott, and M. Lehning, “Spatial and temporal variability of snow depth and ablation rates in a small mountain catchment,” *The Cryosphere*, vol. 4, no. 2, pp. 215–225, May 2010, doi: 10.5194/tc-4-215-2010.

   S. P. Anderton, S. M. White, and B. Alvera, “Evaluation of spatial variability in snow water equivalent for a high mountain catchment,” *Hydrol. Process.*, vol. 18, no. 3, pp. 435–453, Feb. 2004, doi: 10.1002/hyp.1319.

   G. Jost, M. Weiler, D. R. Gluns, and Y. Alila, “The influence of forest and topography on snow accumulation and melt at the watershed-scale,” *Journal of Hydrology*, vol. 347, no. 1–2, pp. 101–115, Dec. 2007, doi: 10.1016/j.jhydrol.2007.09.006.

   E. J. Smyth, M. S. Raleigh, and E. E. Small, “Improving SWE Estimation With Data Assimilation: The Influence of Snow Depth Observation Timing and Uncertainty,” *Water Resour. Res.*, vol. 56, no. 5, May 2020, doi: 10.1029/2019WR026853.

5. Line 39 – this is an awkward description: “Snow depth, used to infer SWE when integrated with snow density”. Rephrase this in a more straightforward way.

   This clause is now redundant given the sentence added in response to comment 4 (above) so the clause has been removed. It now reads: *“Snow depth has been determined …”*

6. Line 52 and 61, – The term ‘grain-size’ is out-dated. The term ‘microstructure’ is preferred. Also, be specific and refer to it as ‘snow microstructure’. This is

important because further in the paper you refer to ‘soil microstructure’. It will help avoid confusion.

The suggested change has been made.

7. Line 55-56 - awkward phrasing – rephrase using common expected terms like ‘backscatter’. Try eg '...interpreting variation in backscattered radiation following interaction with the snowpack.' It seems in this sentence you are trying to do two things: 1.explain how a SAR works and 2. explain how it is used for SWE estimation. In reality, you should only be explaining 2. If you want to explain how a SAR works (ie. it transmits, and then receives backscatter), then you should do it earlier on.

   Agreed. The text has been changed as suggested to: “*In contrast to passive systems, active-microwave SWE estimation methods are based on interpreting variation in backscattered radiation following interaction with the snowpack.”*

8. Line 72-73 – confusing and awkwardly phrased. Also, I’m not sure what is meant by ‘spatially inhomogeneous changes to snow distribution. Should be reworded in a more plain and straightforward manner. Suggest something like: “Decorrelation increases with liquid water content, changes in snow distribution, and volume scattering.”

   Agreed. The text has been changed exactly as suggested.

9. Figure 1. Label Inuvik on each panel.

   The suggested change has been made.

10. Figure 1. Is panel (d) really necessary? Do we really care if an area is alluvial or colluvial? I would suggest at least reclassifying to reduce the number of classifications as it is hard to read and not really useful Likewise for panel (c) – there are too many classifications. It is too hard to read, and the additional classes don’t add additional value.

    Regarding panel (d), we disagree with the reviewer on this point. Only 5 surficial classes are shown and they are coarsely delineated (in fact only 6 distinct sub-areas are delineated). Since heave from seasonal ground ice is likely the largest error source for the method, this panel provides useful context for the reader regarding surficial conditions affecting ground ice conditions.

We agree that panel (c) shows too many classifications. We have simplified it to show a reduced number and have also removed the 1968 wildfire margin since this relates to the removed applications section (section 7).

11. Line 112 – how often should a new DEM be generated for this method (ideally)? What are the implications for accuracy?

    That is a good question and relates to scale. Note that the effect of DEM error on accuracy is detailed in the section titled "DEM Error". A note has been added to the text in the "DEM Error" section: *"Regarded requirements for DEM generation frequency to accommodate landform changes, at the scale applied for our study (DEM smoothed to 90 m resolution), DEM generation frequency can likely be > 10 y. However, if applying the method using more finely scaled data, a more recent DEM may be required since fine-scale landform changes occur more frequently."*

12. Line 123 – Fig. 1c doesn't depict vegetation density, only distribution of the vegetation classes.

    Agreed. The text has been changed to: "…there is significant variation in vegetation classification within the study area…"

13. Line 123 – You mention the upland area east of the delta. It would be helpful if this was delineated in Figure 1.

    An approximate margin has been added to Fig. 1b.

14. Line 125 – 127 - What does 'extensively developed lands' mean? I would suggest using an estimate of developed area in sq km, instead. Hard to imagine Inuvik being described as extensively developed.

    We agree that 'extensive' is misleading. The text has been changed to: "*There are 6.5 $km^2$ of developed lands in the study area…*"

15. Line 132 - does this pose a problem when using an older DEM (that could potentially become outdated by land deformation), or using your methods in general, in this area and across much of the low arctic? Why or why not?

    There is some overlap here with comment #11.

    For this to be an issue, the deformation would have to have a significant relative effect on topographic slopes which is unlikely. This likely only occurs with large mass movements, e.g., land slides or thaw slumps which could generate local estimation biases if an outdated DEM is use.

16. Figure 2 - What is snowfall water equivalent? Or do you just mean SWE? This seems a strange metric and I don't know how it was calculated or what it means. Can't you just use snowfall amount? This is what your readers will expect. In order to convert to snowfall water equivalent, I presume you would

need to know the density of the precipitating snow which sounds difficult. This seems too complicated when good old fashioned snowfall amount will do.

Agreed. The figure has been changed to show snowfall.

17. Line 145 – “Surface and volume scattering occur at the air-snow interface and within the snowpack respectively, but for sufficiently dry-snow, it can be expected that these contributions will be much less that the primary ground-scattered return.” Provide a reference.

    This assumption is detailed in Leinss et al. 2015. This reference has been added to the text.

18. Line 146 – 148 and Figure 3 - It may be useful here to mention and label the wave front in this diagram as in Fig. 7 of Leinss et al (2015). This makes it more clear for the reader why you are considering the particular segment lengths in Figure 3.

    Agreed. Fig. 3 gas been revised to show the wavefront at the instant it reaches the snow surface as in Leinss et al (2015) and the text has been revised: “*This is shown in Fig. 3 which depicts both snow-free and snow covered cases which diverge at the point where the wavefront reaches the snow surface.*”

19. Line 153 – Please specify if you are using just the real portion of ε.

    The text has been changed to: “…is via the real part of the relative dielectric permittivity ε”

20. Line 155 - Is this appropriate for the high-density wind slab often found around Inuvik up to 500 kg/m3? What type of snow did Leinss et al (2015) consider? What range of density? Leinss et al (2015) was conducted at FMI in a forest clearing - likely not much wind slab to be found there. This may be worth considering as a potential limitation. If you are going to use this assumption, you need to demonstrate how similar or different the snowpack observed in Leinss et al (2015) was to the snowpack surrounding Inuvik.

    This is a good question, and also raised by reviewer #2. Instead of establishing similar conditions as observed in Leinss et al (2015), we can use the same layered analysis as Leinss but applied to the case of 500 kg/m$^3$ windslab over a low density hoar layer. To simplify the analysis, we consider the limit case of zero density hoar (unrealistic but chosen to demonstrate the asymptotic limit of our argument) overlain by the windslab. For the windslab, we have an assumed density of 300 kg/m$^3$ and an actual density of 500 kg/m$^3$. According to Eq. (19) and Fig 9a (now extended to rho = 0.5) the fractional estimation bias is +2.5% which is still a small error compared to the other bias sources considered in our analysis. Note that in this case the intervening zero density layer contributes zero additional snow phase. This is an extreme example. In reality the hoar layer will have some low but finite value and the fractional bias will be less than this +2.5% limit.

We have added the follow text:

*"Regarding the effect of vertical density layering, our study area is prone to windslab formation where the late season snowpack can consist of dense windslab overlaying a low density hoar layer. Considering the extreme case of near zero density hoar overlain by $\rho = 0.5$ windslab, assuming uniform $\rho = 0.3$ results in a +2.5% estimation bias which is still a small error compared to the other bias sources considered in our analysis."*

21. Line 332 – There have been enough studies around Inuvik (ie. Trail Valley Creek) that you could have generated an average density from real data instead of just assuming a value. Why didn't you use the data available?

    This was not done because of the fact that the method is quite insensitive to density misspecification. The mean relative snow density across all eight transects used for the in situ validation is $\rho$=0.17.

    Our previously noted bias of 1-3% was estimated very approximately. Using Eq. (19), assuming $\rho$=0.3 results in a +2.1% bias which is small compared to the other error sources. The text has been modified to be more exact: *"For simplicity we assumed a value of $\rho = 0.3$ for all ΔSWE estimates. This value is likely too large for the dry-snow season in Inuvik. For example, the mean snow density measured across all transects summarized in Table 2 is $\rho = 0.17$ and according to Eq.(19), use of the assumed value results in a 2.1% bias which is small compared to the other errors discussed in Sect. 5."*

22. Line 170 – Define what is meant by 'local' with respect to a local spatial region? Is there an associated scale? What would be an ideal scale? Why? Make sure to support with references.

    At this point in the manuscript we are not assuming any scale, and are simply developing the concept that a spatially constant SWE change will result in spatially variable dry-snow phase due to the spatial variation in $\xi$. This concept is not tied to a particular scale, although validity of the assumption certainly is, but this is dealt with later in the manuscript. The following text had been added: "The validity of assuming a uniform snow layer under a local window and selecting an appropriate spatial scale are discussed in Sect. 5.2 and Sect. 6.6"

23. Line 180 – 'Sensitivity of the dry-snow phase' seems slightly awkward. In the caption of Figure 5, you call it dry-snow phase sensitivity. This seems slightly

better. More to the point, you should use a consistent name for these variables throughout.

The text has been edited to be "*dry-snow phase sensitivity*" consistently throughout the manuscript.

24. Line 183-185, Eq (4) – With eq (4) and all of your equations, you introduce them inline, within a sentence. This gets confusing, especially within complicated sentences and makes it difficult for your reader to establish the name of each variable being calculated. I'm not quite sure what ξ is actually called. It would be helpful to include the variable symbol in brackets next to each variable name in a sentence. I strongly suggest you introduce each expression in a more straightforward way, for clarity, such as: 'then the spatially variable sensitivity of the dry-snow phase to a uniform SWE layer (ξ) can be computed as in Eq.(4).

    This change has been made as suggested for this and other similar cases where a new symbol is introduced inline with an equation (Eqs. 1, 4, 6, 16, 17, 22, 23, 24 and 28).

25. Line 182 – you tend to introduce variable symbols, but then continue to use their name instead of the symbol. An example here is snow density (ρ). You have already introduced this variable earlier on, and you throughout the paper you continue to refer to it as snow density despite introducing the symbol ρ. You do this with a number of other variables throughout. In doing so, there are also cases where you refer to a variable by slightly different names which gets confusing. Go through your entire paper and make sure you introduce variables once, and then refer to them by the symbol thereafter. This should irradicate instances where you use different names for the same variables. I will try to point out other cases of this, but it will not be an exhaustive list, so I will leave it to you to go through your entire paper.

    We have gone through the entire manuscript and replaced descriptions with their symbol wherever possible. However, we have left some descriptions in figure captions and the conclusion section to allow a casual reader to gain an understanding without reading through the text to find the symbol definitions.

26. Line 183-184 – another case where you've introduced a variable and expression inline with the text. It is difficult to understand is $\Phi_s$ interferometric phase contribution, or topographic sensitivity? There should be no ambiguity here. This should be rewritten for improved clarity. Put the symbol in brackets next to the variable name in the sentence.

    $\Phi_s$ is now introduced explicitly just prior to Eq.1.

27. Figure 4 - is this a realistic depiction of how snow accumulates on slopes? What about drifting and accumulation on the leeward side vs. windward side? does $\alpha_1=\alpha_3$ ? It's not apparent in the figure. I don't think Figure 4 is mentioned in the text. Please describe in-text. Did you discuss the change in ξ from foreslope to backslope? It is depicted in Figure 4 and should probably be mentioned in the caption.

Spatial uniformity of the SWE change within the estimation window is assumed by the estimator and this figure depicts the estimation model. Of course, this will be violated to some degree and this is discussed both during the estimator derivation as additional bias terms (see Eq. 13) and also in detail in the section titled "Violation of ΔSWE Horizontal Homogeneity Assumption".

The caption has been expanded to (1) mention the increase in ξ; (2) mention that the depicted uniform snow layer corresponds to a method assumption; (3) indicate that $\alpha_1$ and $\alpha_3$ are not assumed equal; and (4) that the figure depicts, for illustration purposes, the special case of SAR line-of-sight and slope normal being coplanar with the vertical which is not assumed.

28. Line 190 – You write: "According to Eq.(5), if the absolute unwrapped dry-snow phase, Φs, can be recovered, then the spatially varying SWE change can be directly estimated at the same spatial resolution as the interferogram." You have already introduced the variable $\Phi_s$ in the text surrounding Eq.5 so you should just be using the symbol here. Furthermore, I was confused about what you called $\Phi_s$ from your ambiguous description in the text surrounding Eq.5, but now I am even more confused because here you call it "absolute unwrapped dry-snow phase." This was certainly not what you called it earlier. This is similar to my comment about Line 182 which emphasizes the need for you to carefully review the manuscript for these ambiguities.

This text now just uses the symbol, Φs since it was already introduced previously just prior to Eq. (1) as: "*The unwrapped phase of the SAR signal due to the dry-snow layer (Φs)…*".

The manuscript has been reviewed for ambiguities related to defined symbols and where possible, just the symbol has been used. In instances where text is used redundantly for clarity, we checked for consistent usage.

29. Line 197 – Similar to my question #22. What is the size of 'local' estimation window to which you are referring? Quantify 'sufficiently large' or give some recommendation of appropriate size. The spatial window is also discussed on line 228. Be sure that your quantification of local window is appropriate for each use in the manuscript.

We thank the reviewer for pointing this out and see how the reader has been given no clue about the scale being used/proposed. The approach used was to start general and then only get specific later in the manuscript but agree that this is unhelpful. The text has been changed to be simply: “Consider a local estimation window, e.g. 1 km × 1 km.”

The issue of “sufficiently large” is dealt with later in the manuscript and need not be mentioned at this point.

30. Eq (9). Check that has been defined. It seems like you define it on Line 221, but it should be defined here.

    The definition has been moved to immediately follow Eq (9).

31. Line 217 – Provide an explanation and a physical basis for this assumption.

    Our wording is misleading/incorrect. For example, when <ΔS> is zero then the first term will be zero, and certainly not dominant. We were trying to say that the additional terms contributing to the bias need to be sufficiently small. This is already expressed in the subsequent paragraph, “The estimator relies on the assumption that the horizontal SWE change distribution is uniform within the window and that other interferometric phase components are uncorrelated with $\tilde{\xi}$”. The text has been changed to “Assuming $\tilde{\Phi}$ correlates with $\tilde{\xi}$ with the proportionality <ΔS>, we introduce the following correlation-based estimator for <ΔS>”.

32. Line 220 – you’ve already defined . Just use the symbol here. Note your description of on line 220 is slightly different from how you described it in Eq.(7). The definition on Line 220 seems clearer, so it should be adapted for use in the text describing Eq.(7).

    Agreed. The text has been reduced to: “This estimator correlates $\tilde{\Phi}$ with $\tilde{\xi}$ and relies…”. Note that $\tilde{\Phi}$ is first introduced in Eq. (6) so that is where we put the description rather than Eq. (7).

33. Line 221 – you should have already defined while introducing Eq.(9). You should just use the symbol here.

    The suggested change has been made.

34. Line 224 – You have already defined on line 210. Just use the symbol here. Note, your definition of on Line 224 is clearer and more straightforward than what you’ve written on Line 210. I suggest you replace the definition on 210 with that on Line 224.

The suggested changes have been made.

35. Line 228 – You have already defined . Just use the symbol here.

    The suggested change has been made.

36. Line 244 – what are the implications associated with ΔSWE = 28 mm?

    This implies an estimated spatial mean SWE change of 28 mm under the estimation window for the time span 2012/03/17 to 2012/04/10 using the SlopeVar estimator.

37. Figure 6 – Is 6d a histogram? it looks like a scatterplot. If it's a scatterplot, can you provide some statistics to quantify this association?

    It is a histogram as stated, however no colour bar was shown. There is one data point per pixel in the estimation window and so a scatterplot becomes saturated when used in this way which is why a 2D histogram was used. The association is derived from the periodogram optimization as noted in the figure caption. A colour bar for the histogram frequency has been added.

38. FIgure 6 - Use consistent headings and symbols in the figures. Eg. if 6d is based on 6a and 6b, then use either the same symbol, or the same name. If the x axis of 6c is the same as the heading of 6a, then it should be written the same way (use either just the symbol, or the same name). Similarly, for the y-axis on 6c, if it is meant to be the same as the title of 6b, then write it the same way (use either centered phase OR uncorrected phase, but not both). Be consistent.

    We have gone through and made these consistent. They now use the same symbols and are consistent with the caption text.

39. FIgure 6 - IF the axes on 6c are not the same as 6a and 6b, why not? you indicate a correlation between 6a and 6b but then show us a different relationship in 6c in order to demonstrate the correlation? This should be changed to a scatterplot of 6a vs 6b.

    Sorry for the confusion. The y-axes of 6b and 6c were omitted and this created some confusion with the colour bar labels from 6a and 6b. The x- and y- axed on 6a, 6b and 6c are all the same (Ground range and Azimuth position) and are now labelled as such. Fig 6d shows the joint relationship between 6a and 6b as a 2D histogram (see comment #38).

40. Figure 6 - It's hard to see a good correlation between 6a and 6b. in 6a I see two areas of high SWE sensitivity - yellow patches at mid-right and top right. In Fig 6b, these seem to match with a heavily speckled area (top right), and a largely

pink area (mid-right). It seems maybe some inconsistency in SWE sensitivity. Can you quantify the correlation? It's not clear that it is a great correlation, and having it quantified would allow us to put appropriate weight to these results. I imagine that's why you chose not to say 'a strong correlation'

First, in accordance with comment #39, we rescaled the 6a colour bar to have the same range as the x-axis on 6d, just to keep everything consistent. This slightly changes the appearance of 6a and it is no longer saturated at the upper and lower ends which is better for comparison with 6b. The heavily speckled areas correspond to low coherence areas which reduce the overall correlation level but do not indicate systematic sensitivity differences. The correlation for this window is 0.34 which is a 'moderate' level of correlation. This has been added as an annotation to 6d.

41. Lines 255-256 – It would be useful to provide a little more information, even though you have provided a reference. Why do non-sequential interferograms provide additional information, and what information do they provide? I don't think you need too much detail, but just expand the sentence a little.

    The text has been changed to: "However, for distributed scatterers which predominate in natural terrain, additional interferograms (e.g. 48-, 72-day, . . . ) are known to allow for reduction in the variance of estimated sequential phases using a phase-linking estimator (e.g., Guarnieri and Tebaldini, 2008). This is because such scatterers exhibit complex Gaussian scattering statistics which are not fully characterized by the set of sequential interferograms alone."

42. Line 263 – Cubic interpolation – Why was this method chosen? Did you try any other methods? Justify your choice.

    We did not thoroughly investigate the issue of most appropriate interpolator and so there may be room for improvement here. The following text has been added: *"We did not thoroughly investigate the issue of most appropriate interpolator and so it may be possible to reduce these errors by using a different interpolator."*

43. Line 263 – blunders in the raw DEM – What does this mean? What blunders occurred, and what were their magnitudes?

    We observed two narrow (few DEM samples wide) but extended (kms long) linear features running diagonally across the computed slope maps, approximately perpendicular to each other. They may be 'stitching' artifacts resulting from mosaicking within the DEM product. We did not quantify their magnitudes but note that they are not obvious when rendering the DEM as height values, and are only apparent when rendered DEM as a 'shaded' DEM or when rendering the $\xi$ parameter, which is slope derived and therefore sensitive to spatial gradients.

44. Line 265 – smoothed DEM to 90 m resolution - If lateral heterogeneity of arctic tundra snowpack peaks at 100 m according to Sturm and Benson (2004), I wonder what potential error smoothing to a spatial resolution of 90 m may introduce given that local scale variability <= 90 m may be missed in the estimation since the smoothed DEM will not include topography which is influential in the local scale variability of snow accumulation. How would this effect the accuracy of your methods? This could be discussed around Line 559 where you briefly note the method has its limits.

    We thank the reviewer for pointing out Sturm and Benson (2004) which characterizes the spatial scale of snow layer horizontal heterogeneity. These findings support smoothing the DEM to 90 m since correlating $\xi$ to the snow phase at shorter scales will introduce sensitivity to this short scale heterogeneity of the snowpack and lead to additional bias. By using a 90m scale for the correlation, the shorter scale snow variations contribute to phase 'noise' which will increase the estimation variance but not as a bias as discussed in the section titled "Uncorrelated Phase Components".

    The following text has been added to the section titled "DEM Error":

    *"Note that smoothing the DEM has the benefit of reducing potential bias introduced by short scale horizontal heterogeneity in the snow-pack (e.g. those noted by Sturm and Benson (2004)). Instead, the effect of these variations will be limited to an increase in the uncorrelated phase components discussed in Sect. 5.4."*

45. Line 267 – provide the appropriate reference for the permittivity relationship (it's not Leinss et al., 2015).

    The reference has been changed to Mätzler (1987).

46. Figure 7 – what is the grey on each panel? This should be included in the legend or mentioned in caption.

    Water areas and points with no solution in the ΔSWE search range are greyed out. This has been added a note in the caption (and also where appropriate in the other figures).

47. Line 289 – You have already defined ξ. Just use the symbol here. Note the definition on Line 289 is slightly different from that given on Line 212. Verify these are meant to be the same thing.

    The text has been reduced to just using the symbol and we have verified that the usage is consistent with  Line 212.

48. Line 308 – You have already defined ξ. Just use the symbol.

    The text has been changed as suggested.

49. Line 309 – You have already defined ρ. Just use the symbol.

    The text has been changed as suggested.

50. Line 346 - what was the model resolution or grid cell size? How does this compare to topography around Inuvik, and correlation length of snow depth in the region?

    This is a good question. The snow model was run on a 12 m grid corresponding to the TanDEM-X DEM used as topographic input. This is significantly finer than many topographic features within the study footprint as can be seen in shaded DEM, Fig. 1b which shows significant structure at coarser scales.  We are unware of any studies characterizing snow depth correlation length in the region.  Certainly, the model run at 12 m scale will fail to model finer scale modulations of snow depth/SWE.  The effect of such modulations in real data will be some degree of decorrelation in the multi-looked snow phase, corresponding to higher estimation variance rather than a bias.

    The following text has been added: *"We ran the model using the same 12 m TanDEM-X DEM used for InSAR topographic phase correction and SWE change estimation. Therefore, the model did not simulate variations in SWE at scales finer than12 m. Note that spatial SWE variations finer that the resolution of the SlopeVAR input data, ξ and Φ, will not bias the estimates but instead will contribute to phase decorrelation and therefore affect the estimation variance, as discussed in Sect. 5.4."*

51. Lines 356 – 359 - Environment Canada (EC) = Environment and Climate Change Canada (ECCC);  these coords are odd...better written as 68.74°N, 133.54°W; it would be more helpful to show these all on a map instead of descriptions like '43 km north of scene center'; also how was the 3 met station forcing data integrated? eg. if all 3 recorded a different precip amount at a given time?

We thank the reviewer for these suggestions. “Environment Canada (EC)” has been changed to “Environment and Climate Change Canada (ECCC)”. The Longitude format has been changed as suggested.

Each station provides mean daily inputs. SnowModel spatially interpolates the data using a weighting parameter that depends on the inverse squared radius from each station. However, the three stations used are very unequally spaced. Mean radius to Inuvik Climate, South ITH and Trail Valley Creek stations are 6 km, 21km and 43 km respectively. Normalizing according to Inuvik Climate station, these correspond to inverse square parameters of 1, 0.08 and 0.02 respectively. The result is that Inuvik Climate station dominates the model input which is the station already labeled in Fig 1a. Rather than adding another figure to show the locations of these stations with marginal impact on the model results, we propose adding the following text: “*SnowModel weights the station inputs according to inverse distance squared parameter and therefore Inuvik Climate station dominates the model input*.”

52. Line 364 – Already defined ξ. Just use the symbol here.

    The text has been changed as suggested.

53. Figure 10 - in f) it would be helpful to include more graduations on x axis such as -20, -10, 0, 10, 20(if it fits). I see you included more in panel c) so why not here? It would help us understand the data spread > 0

    More graduations have been added: -20, -10, 0, 10. Graduation 20 is beyond the plotted range and so was not added.

54. FIgure 10 - maps in panel a) and d) are too small and hard to see. Figures should be larger. Text in panels b) and e) looks too crowded.

    The maps in panels (a) and (d) have been enlarged. The text in panels (b) and (c) have been reformatted and rearranged to look less crowded.

55. Line 387 – Its not clear to me how we know the spatial pattern is correlated from the topography by looking at Fig. 10a. Please add a little more description to clarify this point.

    We agree that the description ‘correlated with topography’ is misleading in that it implies an actual correlation with height which is not true. We were trying to indicate that there are spatial features generally aligned with topographic variations. The text has been changed to *“…shows mean errors in the +/- 2 mm range with spatial features (Fig 10a) that are generally aligned with topographic variations (Fig 1b).”*

56. Lines 418- 420, Eq(22) – What is $\Phi_{SA}$? Is it 'phase modelled from a simple linear function?' It is not clear from this sentence. What does SA stand for?

    Here 'SA' stands for 'static atmospheric'. The text has been changed to *"…the static atmospheric phase contribution ($\Phi_{SA}$) can be modelled as a simple linear function…"*

57. Line 425 – OK, great! I think this is what I was looking for earlier. You have given an estimate of the window – is this the same as the 'local window' mentioned earlier? This quantification should be provided much earlier, along with a discussion of why that window was chosen and what is the max and min recommended sizes and why.

    A forward reference to Sect. 6.6 ("Spatial Resolution") has been added here. Note that Sect. 6.6 discusses the rationale for window size selection including which factors to consider. Regarding providing this earlier, see response to comment #23.

58. Figure 12 - maps in panels a) b) and c) are too small

    Figure 12 has been enlarged and the grid transposed to provide more space for the maps. They are now significantly larger.

59. FIgure 12 - text in panels d), e), and f) looks crammed in and messy.

    These have been reformatted and more space given between the text.

60. FIgure 12 - seems odd to have the panels for heave and solifluction presented here, and not in their respective sections. At least provide the reader some indication as to where they will be discussed (perhaps in the caption, if nowhere else).

    This was done to save some space and it allows side-by-side comparison of the sensitivities of the bias to these different factors. Forward references have been added to the caption.

61. FIgure 12 - shouldn't the x-axes in panels a), b), and c) be in ground range?

    Yes, the values were in ground range but the label was incorrect. These axis labels and ticks have been replaced with a scale bar to make the plots less 'busy' and give space to enlarge the figures (see comment #59)

62. Lines 458 – 460 – What about annual displacement amplitudes for heavily organic soil (eg. peat)? This is present around Inuvik and other high-latitude

sites. It is an important consideration if you plan to use your methods eg. pan arctic.

This is a good question. The following text has been added:

*"Areas with heavily organic soil (e.g., peat), can have significantly higher seasonal displacements. In the vicinity of the study area, we have observed that these occur in localized areas without significant topographic variation, i.e. areas already not well suited for the method because of limited ξ diversity. It may be possible to exclude these areas by masking based on local ξ diversity or analysis of summer interferograms (we have observed that these areas tend to have low temporal coherence due to their significant displacement phase)."*

63. Line 463 – What is your accuracy target for SWE estimation and why? Did you mention it already?

    We have not mentioned an accuracy target. The goal of the manuscript is to introduce this new method and evaluate its accuracy, both analytically and experimentally without a prior 'target' for accuracy. However, this comment does highlight that we need some means of qualifying the significance of the error.

    The following text has been added *"…which is very substantial, considering that that 24-day ΔSWE values are typically <20 mm for the study area (see Fig 15)."*

64. Lines 468 – You write 'Of course, this is not possible for the case of widespread seasonal surface displacement as is common in periglacial regions." This is an interesting point. What are the implications for pan-arctic implementation? What proportion of, say, Canada's north is periglacial? In other words, how big of a problem is this? Quantify it.

    The following text has been added: *"Such areas are relatively common (e.g., Obu et al. (2019) report that 22% of the exposed land area of Northern Hemisphere potentially contains permafrost)."*

65. Line 472 – Should a snow-free baseline be taken each year?

    Solifluction was considered as a potential bias factor for snow-free estimates which we produced as validation 'true-zero' controls. Such controls would, in general, be useful for characterizing the contribution from non-snow bias sources but are not required by the method.

66. Line 484 - What is the physical meaning behind this scaling factor? how is it determined? What is the acceptable range of values and how much does it

affect the outcome if using the min vs, the max value? why can we assume it is constant over the window - provide support for this - references perhaps?

This scaling factor corresponds to the heave amount, less any retrograde motion that occurs due to soil cohesion. Valid ranges vary from zero to whatever the heave magnitude is which as noted in Sect. 5.3.2 can typically be tens of mm. Assuming it constant over the window assumes constant heave conditions which we also assumed for the heave analysis. Based on this, it makes better sense to align the heave and solifluction reference cases to use the same constant reference heave amount of 10 mm. This section has been revised accordingly. Eqs. (24) and (25) were needlessly complicated by introducing a velocity and time baseline. These have been combined into a single downslope displacement term $d_{sf}$. The result of this alignment of the heave and solifluction reference cases is that we have modified our conclusions to say that solifluction is not a significant bias contributor (since it contributes only a small fraction compared to that from the direct heave component).

The conclusions text now reads: *"We found that static atmospheric delay, soil moisture and solifluction are likely not significant sources of bias and that surface deformation due to heave, if present, may lead to substantial estimation bias, generating ΔSWE errors at scales comparable to the deformation magnitude.".*

Regarding whether we can assume $d_h$, is constant, we have added the text to the section titled "Surface Displacement": *"Regarding whether we can assume that $d_h$ is constant over the estimation widow, surface conditions affecting heave can certainly vary at scales shorter than the 500 m window size, e.g. as observed by Lie et al (2012) and this spatial variability will contribute to estimation error to some degree although we have not attempted to quantify this effect."*

67. Line 552 - Is this assumption valid for all conditions? eg. shallow snow, or when delta SWE = 0?

The reviewer is correct. The assumption, as stated is invalid when ΔSWE is small or zero. The intention in this section is to make a first order analysis of the height error contribution, neglecting the 'crossed' error terms involving the $\xi$ error AND any of the bias terms in Eq. (12). The text has been changed to

*"As a first order analysis of this error, we neglect the second order terms involving both $\tilde{\tilde{\xi}}_{\Delta h}$ and the bias terms in Eq. (12)".*

68. Line 573 – 575 – it may help the reader to mention, here, that the transect details can be found in Table 2.

Sorry, the table reference was accidently omitted in the submitted manuscript. It has been added.

69. Line 559 – 600 - Can you describe the topographic variation in terms of surface roughness height, or standard deviation of surface roughness? Trying to get an idea visually, of what this area looks like - perhaps you can get photos of the topography of each the low topo area and the greater topo area - it would help the reader connect the values in the rightmost column of table 2 which is unintuitive, with what's actually on the ground. It is important for the reader to have a clear idea what is meant by low and high topography.

    We agree that such photos would be fantastic for the manuscript but do not have them. We also agree that the standard deviation of $\xi$ is very unintuitive.

    In response to this comment, we computed a roughness standard deviation over the SlopeVar estimation window for each of the transect sites by first computing and removing the best fit plane through the DEM under the window and then computing the standard deviation from the residual heights. For sites A, B, C, D, E these roughness standard deviations are 0.8 m, 0.8 m, 0.5 m, 5.9 m and 3.7 m respectively. For this very limited set, this roughness measure is a good predictor of the standard deviation of $\xi$. These roughness values have been added to Table 2.

70. Line 602 - the error bars are large for sites A, B, and C – the low topography areas - perhaps these areas should be masked out - if they were masked out, how much would your accuracy improve over this study region? what % of area would you have left if you masked out the low-topography areas? Did you account for this while choosing this region for the study?

    This question is already answered in Sect. 6.4 titled "Spatial Distribution of the Estimated ΔSWE". By applying a $\xi$ standard deviation threshold of $> 7.6$ rad mm^-1, the RMSE for the snow-free SlopeVar maps (used as controls) decreased from 21 mm to 15 mm.

    This was not accounted for when the region was selected for study. See our respond to your General Question #1 for our reasons for selecting the study site.

71. Line 743 – perhaps restate the RMSE for each.

    This number of 4x is based on the analysis of Sect 4.4 which derives a direct expression for the relative precision between the Delta-K and SlopeVar methods without computing the individual RMSE values.

72. Line 750 – How did you determine this method works better if total SWE > 150 mm? Was this discussed or demonstrated?

    We did not address this question in the main body of the manuscript and it should not have been included in the conclusions. This has been removed.

Technical corrections:

Line 179 – should this read ‘Eq 4’ ?

No, this statement is referring back to Eq. 3.

Line 618 and 623 – inconsistent use of Fig. and Figure. Check entire document and be consistent. There are more cases – I won’t list them all here.

This was intentional and conforms to the recommended style information described in (https://www.the-cryosphere.net/submission.html): *“The abbreviation "Fig." should be used when it appears in running text and should be followed by a number unless it comes at the beginning of a sentence”.*

Table 2 – Fix formatting of table. Column heading ‘Length’ overruns the column width, and the ‘h’ is on the next line. The last entry in the same column is also too wide for the column and reads ‘Not report ed’. Longitude coordinates, again, seem odd. Usually written as 133.775° W, for example.

The table has been revised as suggested including changing the format of Longitude coordinates.